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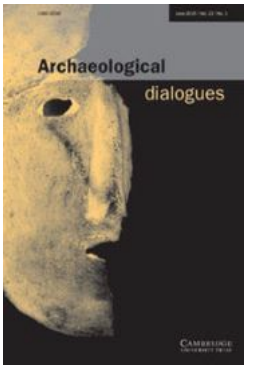

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Excavation methodologies and labour as epistemic concerns in the practice of archaeology. Comparing examples from British and Andean archaeology

*Mary Leighton**

Abstract

Archaeologists' excavation practices vary significantly from country to country and site to site. But variation in the most fundamental, 'common-sense' excavation practices is 'black-boxed' - it is not discussed outside casual, informal contexts, and is treated as having no effect on higher-level interpretation. These practices can, however, be a source of conflict when archaeologists from different communities of practice work together. In this paper, I explore what variation in excavation methodology reveals about the nature of archaeological knowledge itself. By comparing methodologies and the organization of labour on British and Andean excavations, I argue that archaeologists in different communities of practice have divergent understandings of what the object of archaeological investigation is, and of how it can be known, and by whom. This results in contrasting understandings of the nature of material/archaeological objects, as well as contrasting conceptualizations of excavation as an 'expert' practice – one requiring skills, knowledge and bodily practices that are specific to trained archaeologists. Situating these concerns in historical and ethnographic context, this paper suggests that archaeological excavation is, in fact, a far more complex, nuanced and variable practice than the lack of attention paid to it implies.

Keywords

methods; excavation; history and ethnography of archaeology; Andean archaeology; British archaeology; science studies

Archaeology is a unique kind of scientific practice, in part because of its extraordinary diversity. While some archaeologists work closely with artists, poets or writers, perhaps while situated in humanities departments, others see themselves as sharing intellectual and methodological affinities with chemists, forensic scientists or geneticists. Archaeologists might refer to themselves as social scientists, anthropologists or simply 'scientists', or see themselves as

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more closely allied with historians and classicists.[1] This disciplinary diversity has been the subject of some tension over the years, most starkly perhaps during archaeology's version of the 'Science Wars', the debates between processualist and postprocessualist archaeologies that took place towards the end of the last century. More recently, in the US, archaeologists weighed in on the controversy over the American Anthropological Association's dropping the term 'science' from its description (Wood 2010; Berrett 2010; Smith 2010). Such notable controversies aside, however, on a day-to-day basis 'archaeology' includes within its disciplinary parameters a very broad array of methodologies, research questions and disciplinary affiliations without permanently fracturing. When considered from the perspective of science and technology studies (STS) this diversity is remarkable: few scientific disciplines encompass quite such a broad range of intellectual, methodological and theoretical approaches, while still considering themselves to be the same discipline.

If one thing is understood to unite archaeologists, it might perhaps be the use of excavation to gather their primary data – although this statement requires some caveats. In the eyes of outsiders, archaeologists are associated first and foremost with excavation (Holtorf 2006). And while there are indeed many problems with the assumption that all archaeologists excavate (Lucas 2001, 2, in conversation with Tilley 1989), this *is* still the common means of acquiring the most basic data points from which all subsequent analysis proceeds. Without wanting to discount the collection of archaeological material by other means – including survey, ethnoarchaeology, rock art analysis, the analysis of looted material and material in existing museum collections – or to insinuate that the only 'real' archaeologists are those who dig, excavation remains the means through which the majority of archaeological data *first* comes into existence.

The extent to which excavation serves as the bedrock of subsequent archaeological work brings me to a second interesting feature of archaeology when compared with other sciences. While the diversity of disciplinary approaches mentioned above has been a cause of cyclical concern, the vast diversity of excavation methods in use around the world has rarely been a matter of debate or caused the same level of anxiety. Attention was paid to excavation strategy during the late 1960s and the 1970s, as an integral part of the New Archaeologist's critique, but as Gavin Lucas (2001, 2) has pointed out, the subject has not been elaborated on since. Today excavation methods vary enormously between different subdisciplines, from country to country, site to site, and even from person to person within the same project. My contention in this paper is that this variety is important to STS for the simple reason that it is *not* important to archaeologists. In other words, from an STS perspective, what is fascinating about archaeology's vast array of different excavation methodologies is that there *is* no single standard, nor much attention paid to the variety, and this in itself is *not* a problem for the discipline. When compared with other (social, natural and humanistic) sciences, the fact that it appears not to matter *how* archaeology's objects of knowledge come into being is remarkable.

By excavation methods, I mean the most basic nuts and bolts of how one works in the field: what tools are used; what soils, artefacts and ecofacts are

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collected; when something is written down on a form or in a diary (or not); how delineations are made between one object and another; *what* exactly is being measured, scraped, prodded, shifted, counted and collected; and *who it is* doing all this physical, social and intellectual work.

What I am *not* referring to is the kinds of technique usually explained in textbooks on excavation methods given to undergraduates, nor the kinds of strategy written up in the methods section of a site report, dissertation, grant application or article. Neither am I referring to methodological decisions that relate to qualities of the field site itself: for instance, different approaches to excavating a cemetery versus a building, a test pit versus an entire field. Instead, I am interested in the practices that are so habitual they are seen as matters of 'common sense' no matter what kind of site they are used on; or that are only brought to anyone's attention when one archaeologist comes across *another* archaeologist who doesn't seem to be doing what the first would consider to be 'standard practice'.

A few examples of practices that are standard in some archaeological communities but not others could include the use of screens to recover artefacts (standard in the US, uncommon in the UK); field schools where undergraduates without prior archaeological training undertake the majority of the excavation (standard in the US educational system, considered bad practice in Chile); pedestalling artefacts (varies by both country and the time period studied, e.g. it is common in classical archaeology); the use of field diaries; digging in arbitrary versus natural levels; not recording negative contexts; and so on (these last three examples I will explain and discuss in more detail below). The use, or not, of such methods is rarely mentioned or explained in articles, reports or books because they are assumed to be *so* basic that they are not worth describing. But in less formal contexts – for instance when archaeologists chat in the corridor at a conference, or while waiting outside a classroom, or when standing observing someone else's excavation – lapses from what are considered to be basic, 'common-sense' practice can become the subject of professional confusion, gossip or censure.

There are three points to take from this. First, that these are the kinds of practice that are so habitual that they are rarely explained except to novices, and tend not to be discussed in texts. Second, that because they are rarely discussed, they are also unlikely to be formally compared or standardized; as a result, there is substantial variation in basic methods among different archaeological communities around the world. And third, when variation *is* discussed it tends to carry a negative value judgement, but this does not affect the reliability of textual knowledge claims that are subsequently based on that excavation. For instance, a US archaeologist visiting the UK might consider it 'scandalous' that British archaeologists do not use screens to collect their artefacts, but he is unlikely to see this as a reason to dismiss as unreliable everything his British colleagues have ever written.

My contention in this paper, therefore, is that these small differences in habitual, basic practice do not affect interpretive claims at the higher level beyond the field site itself – in other words, they do not affect archaeologists' evaluations of each other's knowledge claims.[2] But looking at these differences ethnographically – at the level of day-to-day practice on

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excavation sites, in classrooms and in conferences halls – tells us something significant about the nature of archaeological knowledge itself. I studied excavation methods from different archaeological communities as part of a larger ethnographic project that explores transnational collaborations between archaeologists from the global North and South (Leighton 2014). When looking at archaeological practice cross-nationally, it becomes clear that basic, habitual excavation methods are fundamentally ontological and epistemic concerns. How excavation is carried out reflects what an archaeological object of knowledge is considered to *be* and *how it is knowable*, within a specific archaeological community. Moreover, the consequences of the variety of excavation methods – and thus the variety in understandings of the epistemic basis of archaeological practice – are significant when we look at the global circulation of knowledge claims based on locally contextualized methodological practices, or in situations where archaeologists from different communities of practice work together.

This does not necessarily imply, however, that standardization is *required*. The lack of attention paid to excavation methods does not make archaeology any less robust or rigorous a science. To be clear, my argument in this paper is not that, somehow, archaeologists *ought* to be standardizing their methodologies. In fact, the larger research project out of which this paper arises makes the case that field sciences and social sciences like archaeology challenge existing STS models of scientific knowledge production, precisely because the scientific authority of a specific fact or claim made by an archaeologist is not derived from standardizable practices or objects of knowledge, nor from the institutionally bounded lifeworld of a laboratory, as is the case in the laboratory sciences traditionally studied by STS (Leighton 2014).

When I say that archaeologists are not paying attention to excavation methods, what I mean is that they are 'black-boxed' (Latour and Woolgar 1979, 51). Black-boxing refers to the extent to which a scientific knowledge claim needs to be justified and explained within a specific scientific community. For instance, when radiocarbon dating was a new technology, its use was a matter of uncertainty and debate, but today an author writing in an archaeological journal does not need to explicitly convince his or her readers that C14 is an appropriate way to date an archaeological layer. Archaeologists no longer have to explain, justify and prove the concept of radioactive decay every time they want to make use of a C14 date in a paper, and in this sense C14 dating has become a black-boxed 'matter of fact' rather than an open problem one needs to think about, pay attention to and justify explicitly.

The same is true for excavation methods. When an archaeologist makes a knowledge claim, they do not explain how, at the most basic level, the data supporting that claim came about as a result of specific practices during an excavation. Formal accounts of archaeological work such as conference presentations, journal articles or academic books do not need to describe in detail how the excavation was carried out. The *interpretation* of data might be called into question – at a conference, for instance, presenters might include extracts from their artefact databases to support statistical arguments about the significance of distribution patterns. But they would not show

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images of the context forms or field diaries kept by the excavators to prove that the site was indeed recorded; and it is unlikely that there would be questions from the floor about whether the artefacts were excavated, washed and counted, and, if so, whether this was done by workers, professional archaeologists or students. What this kind of black-boxing speaks to is the nature of 'trust' within an epistemic community, by which I mean the extent to which knowledge statements within a community of experts (such as a specific scientific discipline) are accepted as stated or open to debate (Collins 2001; Shapin 2008). Individuals within a particular scientific community share an understanding of which questions are open or closed, and thus of what kinds of statement can be 'trusted' as stated rather than interrogated (Knorr-Cetina 1999). Moreover, they recognize *each other* as having shared means of warranting and justifying knowledge claims, and a shared ability to do so through membership in the same community: they recognize each other as being 'experts' (ibid.). In archaeology, the interpretation of the significance of artefact distribution patterns is an 'open' problem that one might expect a colleague to challenge, but how those artefacts were excavated is not – it is a closed ('black-boxed') debate that other archaeologists will take on 'trust' in most professional interactions.

My intention, with this paper, is to demonstrate that archaeologists working in different communities do not, in fact, share the same set of basic methods. To make this point, I will contrast two archaeological communities. The first is an excavation project in the Bolivian Andes that I have been researching ethnographically since 2008. The second is the case of British archaeology, as understood through the work of historians and ethnographers of archaeology such as Gavin Lucas, Matt Edgeworth, Paul Everill and Thomas Yarrow. In comparing the Bolivian and British examples I will make the case that there is significant variation in 'standard' or 'common-sense' excavation practices. While some variation can be explained as a result of difference in the material studied, I will be concentrating on those that arise as a result of different histories of practice in each country. Specifically, I will pay particular attention to the different histories and traditions of seeing archaeological excavation as a form of *expert and professionalized labour*.

By expert labour, I mean the extent to which the *skills*, *knowledge* and *bodily practices* necessary to excavate are considered to be relatively simple and inherent within any human body, or whether, conversely, they are considered to be specifically *archaeological* skills that require active training as an archaeologist (whether that training takes the form of a field school, a university course or an apprenticeship). If they are inherent, then there is no reason why people who have not been trained as archaeologists (such as novice undergraduates, casual volunteers or workers) should not excavate. Conversely, if excavation requires specifically expert ways of seeing, touching, talking or conceptualizing material objects, or academic knowledge of the archaeological literature, then it is something that requires trained archaeologists.

British and Andean archaeology have contrasting understandings of the extent to which excavation is an expert or non-expert practice. This is coupled to a different set of methodological practices, which also imply a different

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level of 'trust' in an excavator's ability to see/know the object of study. I will argue, in turn, that the different combinations of labour/methods stem from contrasting conceptualizations of the archaeological object of study either as inherently and immediately knowable, or as an entity that is only produced through expert, tactile engagement.

The structure of labour relations on an Andean excavation

On the North American-run archaeological projects in the region of Bolivia I studied, excavating is physically carried out by indigenous workers rather than archaeologists. This arrangement is stipulated by the indigenous communities themselves: without the economic benefits of archaeological employment, local communities would be unwilling to grant permission to excavate on their land. Excavation projects are therefore organized around a complex and hierarchically structured labour force that consists of both trained and untrained indigenous workers, and Bolivian and North American archaeologists.

The co-directors of the Andean Project,[3] Olivia, Emily and Chloe, were North American archaeologists heading up a team of seven Bolivian and four North American archaeologists (not including myself). Officially, all foreign-run projects in Bolivia have a Bolivian co-director called a *contraparte*. This role was filled by Pablo, a Bolivian archaeologist who had his *licenciatora* degree and was thus senior to the other Bolivians who were still officially students.[4] But in practice the *contraparte* role is mostly symbolic and Pablo was not more involved in decision-making than other archaeologists.[5]

While the archaeologists were not formally distinguished from each other beyond the division between directors and everyone else, the workers were arranged and divided into formal hierarchies. These hierarchies arose in large part because of the way the community that I refer to here as the *comunidad*, with which the project worked, preferred to organise itself.[6] The 14 archaeologists were outnumbered on site by the workers, who numbered between 50 and 60 at any one time. The majority of these positions were rotated – meaning that each week a different person took on each role – but roles were also highly compartmentalized: each person has one very specific task.

Workers were divided into groups of four or five people, known as *quads*. Each quad was headed by a *maestro* – a term that roughly translates as 'master' and signifies a person with superior skill and in some cases decades of experience working for different archaeological projects. There were six excavation quads, three quads connected to the lab and a quad in charge of archaeobotanical sampling.[7] Like other Aymara communities in highland Bolivia, the *comunidad* was divided into four *zonas* that relate to hierarchical arrangements of kinship and geography (Sammells 2013; Swartley 2002). The excavation quads were divided equally between the four different *zonas*, so that each quad was made up of members of the same *zona*.[8] A final position was that of the *facilitator*, an individual who served as a go-between for the archaeologists and the *comunidad*: negotiating, making deals and solving problems as they arose.

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As the only people with positions that required a significant amount of skill and experience, the *maestros* held the only positions that remained permanent throughout the field season. After a great deal of negotiation, however, it was agreed that the *contra-maestro*, the *maestro*'s assistant who was in effect being trained to become a *maestro* himself, would also become a permanent position. All other positions changed every week and it was the *comunidad* (through the heads of each *zona*) who decided who would work. The result was a constant turnover of staff, with new people arriving each Monday morning.

The rotating worker system was a source of some tension. The archaeologists would have preferred to prioritize employing people with experience, or at least enthusiasm and/or physical fitness (they would also have preferred that the more prestigious and better-paid *maestro* roles were open to women). For certain roles that required specific skills and experience, such as processing archaeobotanical samples, the archaeologists wanted to hire particular individuals who had been trained over several years for that role. But from the *comunidad*'s perspective, circulating jobs was the fairest means of ensuring that everyone benefited economically from archaeological employment (although in practice there was a certain amount of political manoeuvring to ensure that some families got a larger share of more desirable jobs than others). Pragmatically, the archaeologists accepted this situation because they had little choice about the matter, but on the whole it also worked very well.

The excavation quads were made up of five people: a *maestro* and *contra-maestro* (always male), a *bucket carrier* (male or female) and two *screeners* (nearly always female but occasionally an older man). Each excavation quad was under the control of an *excavation archaeologist* (male or female). The excavation *maestros* were the most prominent men on site, in some cases with decades of experience working on many different archaeological projects. The *contra-maestro* worked as an assistant to the *maestro*, excavating alongside him with a trowel as necessary. The bucketer's job was to ferry soil backwards and forwards between the excavation unit and the screeners, who waited next to a series of large wooden sieves arranged on the side of the excavation area. The large sieves, mounted on tall wooden tripods and known as screens, filtered the soil removed from the excavation. The screeners sifted through the objects left in the sieve after the soil was shaken out, looking for artefacts that they then sorted into bags of lithics, bones and ceramics.

How to make sense of this large catalogue of people and positions? One way is to think about it from the perspective of an artefact as it moves through all these hands (cf. Holtorf's 'life history of a sherd' approach (2002)).

The *maestro*, with his *contra-maestro* nearby, excavates an area of soil while his archaeologist stands watching and writing notes. Often archaeologists work in pairs so that one can be watching or helping with the excavation, while the other writes notes and fills in forms and tags. As the *maestro* carefully pulls his trowel over the ground, loosening and removing the soil in front of him, he pushes it to the side. The *contra-maestro* leans forward to sweep the now loose pile of earth into the waiting bucket with a plastic dustpan. The bucketer is crouched a few feet away, watching intently

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and occasionally chipping into the conversation between the *maestro* and *contra-maestro*, or looking around him with some boredom. As the bucket is filled, the bucketer picks it up (putting an empty one in its place). He walks briskly over the space of the excavation, climbs up the side to the natural ground surface, and goes over to where his two screeners are waiting. Getting to their feet and interrupting a conversation they had been having with the adjacent screeners, these two young women pick up the large trowels beside them and steady the screen as the heavy bucket of soil is emptied into it. The bucketer stands for a few moments chatting with them, then remembers to turn back a number on a spiral of small, numbered cards nailed to the screen – a system of recording how many buckets of soil have come out of a particular area and thus the total volume removed. The women shake the screen and bash it with their trowels, forcing earth through the quarter-inch mesh and in the process creating a mound of loose soil underneath them that grows to a height of several metres by the end of the field season. When their field of vision is cleared somewhat, they start rapidly picking out fragments of bones, ceramics and lithics with their fingers, putting these into cotton draw-stringed artefact bags.

After a while one of their archaeologists comes over and collects the bags that are full, replacing them with empty ones that are already labelled with handwritten tags. The archaeologist exchanges a few words of greeting with the screeners, then heads back to a spot on the other side of the excavation unit where the archaeologists have left their backpacks and notebooks. There is also a pile of small paper tags and permanent marker pens here. The archaeologist sits cross-legged on the ground and spends a considerable amount of time filling in three identical tags for each bag, double-checking the details occasionally on the clipboard of handwritten notes she has in front of her. She's joined by another archaeologist from another quad as she does this, and as they do this tedious but essential task they pass the time chatting.

At the end of the day the bags are taken back to the lab by the archaeologists and checked against a master database that is presided over by Natalie, a North American archaeology graduate student. Olivia's aim in handing over much of the responsibility for the laboratory to Natalie is both to free up her own time and to train Natalie to run her own projects in the future.

Natalie also oversees the storeroom *maestros*, to make sure that they are keeping track of each bag as it comes in and out of the lab. The next day the bags of artefacts are emptied out by the washers, who dunk each artefact into water, scrub it with toothbrushes, then leave it on a tray to dry in the hot sun. After they are dried, the artefacts are sent in their original bags to the markers, who write a code number on each piece of bone and ceramic with a fine permanent marker pen, before they are bagged up again and taken back to the store room. At some future point, the storeroom *maestros* will bring these bags out to be analysed by one of the archaeologists working in the lab, who will enter individual measurements for each ceramic shard or bone into a database. It may be weeks or even months from now before this happens, however. In some cases the bags can wait in the storeroom for years if there

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is no one interested or able to do the analysis. But on this particular project the directors have graduate students lined up to work on all the material the same year.

In contemplating this complex organization of people and things, involving the circulation of hundreds of forms, thousands of tiny objects, and around 60 new people each week, Olivia joked with me one morning that it worked like 'a well-oiled machine'. This became something of a catchphrase, seeming to capture the sense of precision and concentration required to keep everything moving along. And it highlighted how it was this machine, as a collective agent, that generated all the data that any subsequent analysis would depend on. But the machine or factory analogy is also fitting as a means of understanding the implications of the divisions of labour within this system – the extent to which everyone had a specific role that fits like a cog into the bigger structure. Knowledge was generated by the machine as a whole rather than by any one individual. As we will see, this stands in stark contrast to the British model, with its focus on the single excavator.

Professional archaeology in the UK

As has been described by ethnographers of archaeology such as Matthew Edgeworth (2003) and Thomas Yarrow (2003; 2006), the majority of excavation in the UK today does not involve workers but rather trained archaeologists. This situation, however, arose out of a very specific set of historical circumstances. While early European figures such as Heinrich Schliemann and Howard Carter gave fieldwork an aura of romance, similarly upper-class men like General Pitt-Rivers and Flinders Petrie consciously strove to develop it into a modern science (Lucas 2001, 7). Pitt-Rivers epitomizes the image of this period: a charismatic English aristocrat, directing large teams of workmen excavating prehistoric barrows found on his own estates. Later Kathleen Kenyon and Mortimer Wheeler's excavation techniques in the 1920s and 1930s reflected a new focus on understanding structures and the layout of individual sites rather than only artefact sequences (Lucas 2001, 37–47). Wheeler was particularly influential in terms of his military-inspired emphasis on order and discipline on site and for being the first to introduce 'site supervisors' who were trained as archaeologists and thus had a chance of eventually becoming directors themselves (Collis 2001, 9).

During the 1950s, a 'revolution' took place in archaeology as a result of concurrent technological and social changes: namely the use of mechanical diggers that allowed larger areas to be worked on simultaneously, and the opening up of higher education which generated an abundance of archaeology students willing to work for little or no money (Collis 2001, 11–16). The shift away from staffing excavations with paid workmen, towards student volunteers and ultimately full-time professional excavators, has been described as a process of professionalizing and democratizing archaeology.

The changing social context of archaeological practice in the late 1950s and early 1960s brought further shifts in methodologies as well as personnel. The post-Second World War boom in construction resulted in a growing number of large, urban excavations that needed to be undertaken quickly,

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but the university graduates who staffed them were increasingly seeking paid careers rather than multiple stints as volunteers. This was also the era of 'New Archaeology' and particularly the interest in devising more systematic research designs. Among the new methods devised during this period, that created by the Winchester Research Unit (WRU), established in 1961, had perhaps the longest-lasting impact. During the excavations at Winchester by Biddle and Kjolbye-Biddle, 'open-area phase excavation' became a new norm and Edward Harris developed the 'Harris matrix system' in tandem with the 'single-context planning sheet' (suggested by Lawrence Keen and developed by Harris and Patrick Ottoway (Harris and Ottoway 1976; Harris 1989, 95; referenced in Lucas 2001, 75)). This tripartite system was a solution to the problem of complex post-excavation analysis in the large, urban, multi-phase excavations undertaken by the WRU.

A Harris matrix is a diagrammatic representation of archaeological events arranged according to stratigraphic relationships that must follow strict rules; a single-context planning sheet is a form-based system of recording archaeological events; and open-area phase excavation is the method whereby all periods of a site are systematically excavated in their entirety, rather than in parts. Open-area phase excavation, single-context planning and the Harris matrix are interconnected parts of the same methodological system. As I will explain below, each depends on the logic of the other, although, as we will see when looking at the Andean example, in practice they are often used separately.

The combination of these methods and technologies, however, enabled or led to *paid skilled excavators* who were responsible for both recording and interpreting the section of the site they worked on. This was in contrast to the large teams of volunteers or student workers overseen by a supervisor who still stood as the sole recorder and interpreter of the data produced, despite the workers now being students or graduates. By the end of the 1970s, and particularly after the introduction of the neoliberalized PPG-16 model of commercial archaeology in the 1980s, nearly all excavation work in Britain was done by full-time, professional archaeologists who had university degrees (Everill 2009), and followed a remarkably standardized combination of the Harris matrix + single-context planning + open-area phase excavation method (now more commonly referred to as the 'MoLAS model' after the Museum of London Archaeological Service).

The hierarchically structured Andean projects described in the previous section relied on the unskilled labour of screeners, bucketers, *contra-maestros*, washers and artefact processes, and the narrowly defined expertise of *maestros*, in addition to a small team of archaeologists. In contrast, by the late 1990s and early 2000s British excavations involved only teams of trained, professional archaeologists, arranged relatively horizontally. Crucially, the horizontal organization of labour in the UK was dependent upon a methodological technique and recording strategy that combined single-context planning, the Harris matrix, and open-area phase excavation. This combination of labour organization and methodology also involves a very particular conceptualization of archaeological objects of knowledge.

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The Harris matrix, single-context planning and the professional expert archaeologist

The Winchester system requires a specific conceptualization both of the knowability of the archaeological record and of the archaeologist as an expert knower. A context sheet and a field diary, for instance, stand in opposition to each other as methods of recording. They assume different kinds of professional 'trust' in the expertise of the person filling them in. As described earlier, I am referring to trust here in the sense that it is understood in the social and historical studies of scientific practice, as it refers to the extent to which claims are accepted as stated or opened up for debate.

A field diary is attached to a single archaeologist who records everything that they (or the workers they supervise) excavate. In many cases the field diary is organized like a journal: each day the archaeologist sequentially records details of the area they are excavating.[9]

The innovation of the context sheet lies in its standardization and the non-sequential nature of its use. A single standardized sheet is created with boxes for specific information about the entity being described and the conditions of excavation, and a large space for interpretation/description. Forms are numbered and each archaeological 'context' on site gets its own form. The archaeologist excavating the entity fills in the form, then returns it to a central file overseen by a site supervisor.

The difference between the diary and the form is thus a shift both in the way the archaeological entity is seen in relation to the rest of the site, and in the way the archaeologist holding the pen is framed in relation to the person holding the trowel. The logic of a diary is that the excavation as a whole needs to be understood from the perspective of a limited number of individual excavators, but these individuals are likely to make mistakes and may need to refer back to their own actions or thoughts over the course of the project. A record needs to be kept of the changing understanding of the site as the work progresses for the sake of both the fallible excavators at the time and the director writing up the project in the future.

In contrast, the WRA method was specifically developed for complex but time-sensitive excavations within which a large number of professionals could work together to get the job done quickly and efficiently without extensive supervision. The logic of a context sheet is that there are many excavators, each of whom can be trusted to understand, excavate, record and interpret what they are working on individually. It does not have an expectation of excavator-error built into it. The process of excavation is thus a matter of gathering up information in real time from these many, separate units of analysis, so that by the end of the project a complete and reliable record of the entire site is already compiled, ready to be interpreted at a more complex level by the site directors.

Alongside a different expectation of the excavator's professionalism, however, this system also comes with a specific understanding of what the unit of analysis is, namely what the 'object of knowledge' is: the thing that an archaeologist considers him- or herself to be looking for, handling and recording. There are three different ways of conceptualizing what is being

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excavated, which we can identify and compare ethnographically by looking at what entities are defined as significant and given numbers, *in practice*, during excavations conducted by different archaeological communities. The terms I use below do not always correspond directly to those used by archaeologists themselves, because some terms (e.g. loci, spits, contexts, levels, etc.) have non-identical meanings in different archaeological communities.

In the first type of situation, an archaeologist will excavate, record and assign numbers to *excavation events*. For instance, an archaeologist marks out a 1 × 1 metre square on the ground and removes all the earth within this square to a depth of 10 centimetres, then assigns a number to this 1 m × 1 m × 10 cm volume of soil. Here the archaeologist is defining and recording an object of knowledge according to *actions undertaken in the present by archaeologists*. This is the case even if this particular patch of soil is considered important because it is recognized as being something that existed in the past, such as a floor surface. So, for example, when an archaeologist removes twenty centimetres of a forty-centimetre-deep midden, and assigns a single number to the area of soil they have removed, this recognizes and records a bounded entity that is created through *the archaeologists' actions* even if this is also understood as being 'the top of the midden'.

In the second type of situation, an archaeologist will excavate, record and assign numbers to *archaeological entities*. Now, for instance, the archaeologist designates a particular area of soil as a trash pit in contrast to the floor surface surrounding it, then removes all the soil that he or she considers to be 'trash pit' as opposed to 'floor surface', and assigns a number that stands for the soil as it was when it was in the ground. The archaeologist would record the final size and shape of the trash pit by measuring the depth of the hole left once the soil is removed, and describe its properties by analysing the soil/artefacts that have been removed. In this case, the archaeologist is identifying objects of knowledge to act upon and record, on the basis of *entities from the past that are still physically here in the present*.

The third scenario is slightly different from the two above, and forms the core of Harris's innovation. It relies on the idea that the objects of knowledge to be identified and recorded are *archaeological events*, which are *material traces in the present of actions that occurred in the past* – rather than archaeological entities (material things left over in the present from the past), or excavation events (actions undertaken by an archaeologist in the present). Events can be either 'positive' or 'negative', depending on whether they leave an accumulation of material behind or an absence of material. The action of *digging* a trash pit, for instance, creates a negative event called a 'cut', i.e. a hole, which is an absence of soil. The action of *filling* that pit creates a positive event called a 'fill', i.e. when the hole is filled in with trash, over time it turns into an accumulation of soil. The archaeologist in this scenario would identify *two* archaeological events and assign them separate numbers. One is the act of digging the pit which is given a 'cut' number, and the other is the act of filling the pit which is given a 'fill' number. The material traces of the cut and fill might have the same recordable dimensions, but crucially they represent two separate events and are therefore two separate objects of knowledge. Each is recorded separately and arranged stratigraphically (i.e. in

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the chronological order in which they occurred in the past, as represented by their material trace in the present) on a diagram called a Harris matrix.

The archaeological object of knowledge can therefore be one of three things. Within different archaeological communities, one or another of these will be considered the 'common-sense' object of knowledge, with a corresponding approach to excavating and recording. Conceptually, it is not possible to combine these approaches. A Harris matrix, for example, relies entirely on the recording and removal of discrete *archaeological events* in stratigraphic order: they cannot be excavated and recorded as excavation events or as archaeological entities.

In practice, however, these three conceptualizations often do overlap and are used in various combinations in different parts of the world, for instance by assigning a number to the boundaries of a pit (recording archaeological entities) but then excavating it in 10-centimetre levels and giving each level a number from the same series (recording excavation events). If the archaeologist then wishes to arrange all these numbers in a single diagram that represents their stratigraphic relationship, the question arises of what the diagram represents – things the archaeologist did, or things from the past. In some cases, multiple sets of numbers are used to represent different things (events or entities), and the archaeologist is required to translate one set into another at a separate stage of write-up. I noted on several of the excavation projects that I studied that such combinations often led to considerable confusion between excavators, or anxiety on the part of individual excavators who were unable to 'make the numbers fit' in the way a director requested.

Coming back to the question of who the excavator is and the notion of trust, we can see why this distinction is important to questions of professionalization and hierarchy. The impulse to record excavation events – to use field diaries that preserve a chronological narrative of the excavation, and to excavate in regular 'arbitrary levels' as opposed to 'natural levels' – rests on a different expectation of the excavator's skill and expertise. Single-context planning requires that the entire context be removed and recorded in one go, given a single number, and taken out entirely so that the context underneath it can then be seen, removed, and recorded in its turn. This is only possible when the individual excavator is understood to be capable of both recognizing the context and removing it without over- or under-excavating. In contrast, routinely digging in arbitrary levels is a way of controlling for anticipated excavator error.

But what do these systems look like in practice? Looking again at the Bolivian and British examples, with their distinct methods of recording and their different organizations of labour, do they in practice have different conceptualizations of the epistemic and ontological status of archaeological knowledge objects?

Forms and numbers in Andean archaeology

Archaeologists on the Andean Project used a recording method that combined forms and field diaries. The diaries were individual and were used to keep an ongoing narrative documentation of their own work. The forms prompted the writer to record standardized measurements and descriptions, and a brief

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interpretation: in this respect they were similar to single-context planning forms. They varied from context forms in that the unit of analysis was called a 'locus' and could refer to either an excavation event, an archaeological event or an archaeological entity. An additional set of numbers called 'feature numbers' was later assigned from the loci to construct a Harris matrix.

The layout of the forms had changed over the years, as it does on many excavations in this region. If we take a genealogical approach to the director–excavator relationship – itself commonly also an academic-adviser–graduate-student relationship – the original versions of the forms could be traced back to great-great-grand-advisers of the co-directors through various branches of their professional family tree. But other innovations arose as individual excavators or directors suggested modifications that were used in other excavations with which they had been involved. In this respect, methodologies are far more dynamic and flexible in these Andean projects than they are in the UK.[10] In 2008, several new features and numbers were added to the forms as the directors adapted the recording system to incorporate the locus and the Harris matrix.

The use of a Harris matrix diagram created through loci and using archaeological workers requires some explanation. As discussed above, the Winchester system involves a combination of the Harris matrix, open-area phase excavation and single-context planning, and it aims to record *archaeological events*. But when talking with different archaeologists involved in the project, the distinction between excavation events, archaeological events and archaeological entities was not clear. One of the directors, for example, argued that the novelty of the Harris matrix and locus system was that it allowed one to leave all the interpretation of the site until *after* the excavation was complete – this, however, is the *opposite* of how the Winchester system was designed. When I asked different archaeologists at this site to describe what a 'locus' was, I received a broad variety of detailed and nuanced responses that reflected on their own practices at this site and elsewhere. But taken together these conversations did not agree with each other, and among the excavators themselves there was some debate about what *were* the units of analysis. This caused some anxiety about the need to produce a diagrammatic representation of the site along the lines of a Harris matrix of 'events' by the end of the season. Neither locus numbers nor event numbers corresponded directly to context numbers as understood in the Harris matrix + single-context planning model, although this was the model being referred back to in the various conversations that took place that season.

Certainly many of the archaeologists I interviewed – not only at this site or even just in the Andes – express doubt and uncertainty about their excavation and recording methods. The anxiety on this project about their system was by no means unusual. In drawing attention to the recording methods at this one site I am by no means implying that the Andeanist archaeologists are 'doing it wrong'. Rather, the more interesting question is how one component of a complex technical and methodological system can be separated out and applied elsewhere, without also importing the related technologies, the organization of labour and expertise that the system relies

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on, and the ontological assumptions that underlie its use. The immediate answer is that in many field excavations it causes considerable confusion, but this in itself is interesting because confusion in the field is not a matter of serious concern to the security of knowledge claims further down the line. Even if archaeologists *were* 'doing it wrong', this *does not* affect the validity or authority of the reports or papers they later write and publish. Because there is no formal discussion of field methods, the validity of knowledge claims at the excavation level is black-boxed.

From labour divisions to the epistemic status of objects

There are further implications to the relationship between the division of labour and recording methods, however, that refer directly to the implicit understanding of what an object of archaeological knowledge *is* and the extent to which seeing it, recognizing it, physically bringing it into being, recording it and interpreting it in relation to other archaeological objects are acts of expertise. In the British model, an archaeologist undertakes all these roles. In the Andes, the vast majority of physical interaction with material objects is done by non-expert workers.

> Thursday, 10.35 a.m.
>
> I am sitting on the side of the excavation unit. Olivia and James [North American director and graduate student respectively] are standing behind me, and in front of us Camila and Trinidad [both Bolivian *licenciatora* students] are taking a level. A moment ago there was a short conversation in English about whether the surface was level – Olivia thought it wasn't, James said it was an optical illusion – and James had suggested putting in temporary datum points so that they could casually measure as they went along without always needing the total station. Olivia, James and I are all watching Camila and Trinidad taking these temporary levels. They are in the excavation unit, about four metres away and a little lower down from where we are all standing on the edge of the unit. Olivia turns to James and tells him that Camila and Trinidad shouldn't be taking the levels, the *maestros* should be doing it. James replies that they are just doing it now, to check, but Olivia says again that they ought to be having the *maestros* taking measurements. This develops into a larger conversation about how the *maestros* should be taking all the levels. 'The idea is that the *maestro* and the *contra-maestro* should be doing all the things they can, to free up the archaeologists to do what only they can do.' The archaeologists should only be doing certain tasks like writing bags and filling in forms, and the *maestros* should be doing all the rest to take pressure off the archaeologists. James walks over and steps into the unit to tell Camila and Trinidad this (although later on in the day I see the two Bolivian women continuing to take levels themselves).

An Andean archaeologist is not someone who shovels, carries buckets, picks artefacts out of a sieve, or regularly engages a trowel with the soil. A good archaeologist is someone who is able to make decisions, to think quickly, who works in an orderly manner while managing a large number of people engaged in a variety of manual tasks, and who keeps on top of

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never-ending bureaucracy involving the processing of bags, tags, soils and forms. As with the task of taking measurements, the delineation of roles can shift, but tends to do so in the direction of an ever-finer emphasis on the archaeologist as someone who writes and manages, rather than someone who undertakes tactile engagement with physical objects. The understanding is that a line *can* be drawn between the act of uncovering material entities (that have an existence independent of the observer) and the act of making an expert archaeological assessment of these material entities in order to generate archaeological knowledge.

For example, the separation of artefacts from non-artefacts in the screen is conducted by rotating workers who receive ad hoc training at the beginning of each week. The fine mesh of the screen and the system of noting the exact volume of earth that goes through it implies precision. As parts of the 'machine', the weekly worker's eyes and hands are indistinguishable from the eyes and hands of anyone else and, unlike the *maestro*, they need little or no training. This task of recognizing and selecting artefacts in a screen is understood to be non-interpretive and based on the physical (rather than archaeological) properties of the object. Physical objects like ceramic sherds and fragments of bone are thus conceptualized as self-evidently knowable. Knowing them is not necessarily an 'archaeological' act. A sherd is thus always physically a sherd: it only becomes an *archaeological* entity through the actions of the archaeologist (assigning it numbers, tracking it into a database, connecting and comparing it through analysis to many other sherds elsewhere).

The way that an archaeologist and a *maestro* work together illustrates this division between interpretive and non-interpretive labour further by showing how non-artefactual objects (e.g. soil features like pits, walls, floors, etc.) are also conceptualized as inherently visible as *material* things but not necessarily knowable as *archaeological entities*. The majority of conversations between a *maestro* and his assigned archaeologist are framed in terms of changes in soil texture, colour or compaction. The understanding is that where the *maestro* sees and describes changes in the earth, the archaeologist will see archaeological entities. The archaeologist and the *maestro* look at the same object and see it differently: one sees a material entity, the other sees the material entity as an archaeological entity. There is an acknowledgement on both sides that they are conceptualizing the same material things differently and that *it is this difference in conceptualization* that makes one person the *maestro* and the other the archaeologist.

For instance, the archaeologist gives an instruction to the *maestro* and this instruction encapsulates the way in which the archaeologist imagines the *maestro* conceptualizes the earth: e.g. 'dig 10 centimetres down' or 'dig out the red soil from the black soil'. The product of the *maestro*'s engagement with the earth then becomes 'the soil 10 centimetres down' or 'the black soil'. But the archaeologist sees this same physical thing as 'level 2' or 'a pit'. The archaeologist's expertise and their ability to make archaeological knowledge comes not, therefore, from an interaction with material objects found in the soil, but from the ability to conceptualize the same objects archaeologically once someone else has defined them materially. The implication of this system

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is that archaeology's objects of knowledge have an unproblematic, obvious *material* existence – expert archaeological training is not required to see, uncover or measure differences in soil colour, compaction or texture. But it takes an archaeologist to understand that material thing as an *archaeological* object.

Although it takes an archaeologist to define an object archaeologically, archaeologists are considered likely to make mistakes: the recording of excavation events, the habitual use of arbitrary layers and the requirement of field diaries implies that excavators require constant monitoring. In this system, then, the archaeologists and the workers are both fallible, but the object itself is assumed to be a point of certainty.

Democracy and trust

And yet there frequently *is* discussion about where entities begin and end in both British and Andeanist archaeology, and here we have to take into consideration the tensions that exist between a scientific model of practice and day-to-day experiences. In the case described above, one of the Bolivian archaeologists preferred to take her own measurements, disagreeing that this task could be given to the workers. She was claiming that she *did* need to see and hold the tape for herself if she was to understand the material thing she was recording – to know it directly through tactile engagement. But the methodological system on this site was structured according to the assumption that objects are inherently knowable and measurable. In addition, when the system incorporates an expectation that excavation is a matter of revealing inherently material objects, the archaeologists themselves can also be interpolated as interchangeable: assigning archaeological meaning comes after and therefore is separate from interaction with an object's materiality, so any archaeologist is equally able to do this. In practice this might not be the case, but in theory this is what the model assumes.

In contrast, a slightly different debate has taken place in the UK concerning the implication of giving individual archaeologists sole responsibility for undertaking both the excavation and the recording of contexts. This debate involves a contrasting discussion about the extent to which interacting physically with objects results in greater or lesser epistemic authority, focusing around the question whether or not single-context planning has actually democratized the archaeological process and/or generated more rigorous archaeological knowledge. Single-context planning has been argued to be more democratic because every excavator on site holds both a pen and a trowel. In the old model, a single director or supervisor filled in the field diary; only one person had the power to inscribe a definitive interpretation of the site, even though it was produced through the labour of silenced and often working-class excavators (Berggren and Hodder 2003; Shanks and McGuire 1996; Farid *et al.* 2003, 24; Collis 2001, 44–45). The counterargument, however, is that despite its assumptions of professionalism, the process of using standardized forms restricts excavators, interpolating them as only one of many interchangeable and standardizable cogs in a machine, and keeping them focused on their individual task rather than on the site as a whole (Lucas 2001, 8–9).[11]

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And yet Matthew Edgeworth's (2003) ethnography of a commercial excavation in Britain in the late 1980s describes how the authority of an individual excavator is more dynamic than these arguments suggest and changes over the course of excavating a feature. During an excavation, the supervisors walk around the site to talk to individual excavators throughout the day. On initial visits, a supervisor will give an excavator explicit instructions, but on later visits – once the excavator has been physically engaging with the context for a period of time – the supervisor will give more credence to what the excavator has to say. The excavator gains authority from being the only one to have *material experience* of the context,

> For it is well-known on site that the person who has actually dealt with the evidence at first-hand, has witnessed the emergence of it through time, and indeed has brought about its emergence through his own actions upon the material field, knows more about that particular pattern of evidence – in a practical sense – than anyone else. The importance of touching and manipulating the evidence, as opposed to merely visually observing it, is illustrated by the way in which supervisors visiting a field invariably pick up a trowel and engage in a brief material transaction … in order to get some additional idea of the all-important 'feel' of the material (Edgeworth 2003, 47).

Writing in the early 2000s, Edgeworth and other ethnographers of British archaeology such as Thomas Yarrow were describing something that had become 'common sense' in the UK as a result of the WRU method of excavating: the only person who can fully understand a context is the person who has physically excavated it. We can thus interpret the single-context planning form as a method that assumes that objects are only brought into being as a result of what Daston and Galison (2007) refer to as expert judgement in contrast to mechanical objectivity: it takes an individual with tacit expertise and professional experience to correctly distinguish objects found in nature, implying a more relational model of scientific practice that involves both a knower and an object to be known. We can situate this in opposition to the field-diary and arbitrary-level model, which implies an object that has an unproblematic and inherent existence that, with some control for human error, can be revealed and recorded by an untrained observer.

Of course, in practice the model is less clear-cut, particularly because, as I mentioned above, there is always tension between what the methodological system assumes and how it is used. For example, witness the Bolivian archaeologist who preferred to take her own measurements, the *maestro* who has worked for generations of archaeologists for twenty years and has a far better understanding of archaeological entities than the novice graduate-student archaeologist, the return to amateur volunteers or undergraduate students to staff British excavations. This suggests that in practice the Andean model does not fit comfortably with archaeologists who, as part of their day-to-day work, feel they *do* need to physically engage with objects to understand them. But at the same time, the British system relies on but ultimately *elides* the tacit and embodied nature of archaeologists' expertise

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through forms, a contradiction that lies at the heart of the debates described by Lucas, Hodder and Edgeworth above. In both systems the mundane difficulty and contingency of archaeological excavation is elided – or what we might better term 'black-boxed' – by methodologies and recording technologies that cannot acknowledge ambiguity or uncertainty.

Nevertheless, the point remains that the two systems encapsulate different understandings of the knowability of archaeological objects and the level of professional trust that can be placed in an individual encountering them. The process of excavating material entities, recording them as archaeological events, and interpreting them later as archaeological entities and/or events, implies that objects exist unproblematically but excavators might not have the ability to know them. In contrast, the British model assumes that even though an excavator might make mistakes, the excavator is still a *more* authoritative knower than someone who only looks at the textual record, because knowing objects *both* materially and archaeologically is a complex process that requires *tactil*e interaction. The object's existence is in question until the excavator, who gains authority through material engagement, is able to know it.

Conclusions

The diversity of excavation methodologies at a *transnational* or *subdisciplinary level* reflects the variety of research traditions within which archaeologists work, but also the different conceptualizations of the object of knowledge that arise as a result of different field practices. The contingencies of the field (as opposed to a laboratory) as an open, uncontrolled space are central to practice. Archaeologists must work with rather than eliminate the field, *building into* their methodologies the social and economic relationships they are expected to have with people encountered there, as well as the weather, the geology, the physical condition of the artefacts and so on. Meanwhile the diversity of practice *between individuals within any particular moment of excavation* reflects the diversity of individual embodied experience of the material world. What this suggests is that the lack of standardized methodologies is not a 'failure' of archaeologists to be 'rigorous'. Instead, it suggests that archaeological excavation is a far more complex, nuanced and *expert* practice than the current (lack of) debate over excavation methods acknowledges.

In this paper, I have drawn attention to some of the ways archaeological excavation methodology becomes an epistemic concern. There are two immediate implications of this discussion. The first relates to the micropolitics of practice when archaeologists from different epistemic communities work together. Because there is little formal discussion of excavation methods, it is sometimes assumed that methods are actually similar from one place to another. Thus when different practices *are* encountered, the first assumption is that *someone* must be deviating from that presumed shared standard. When the social context of the collaboration is uneven, for instance when archaeologists from the global North travel abroad to work with colleagues in the global South, the likelihood is that those who are judged to be 'wrong' will be the colleagues from the South.

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The second point also requires a transnational perspective. Certain academic books and journal articles circulate globally and they work, like Benedict Anderson's (1983) novels and newspapers, to create an imagined *supranational* scientific community, namely a shared community of archaeologists who are all reading the same things, and imagine themselves to be reading them in similar ways no matter where they are in the world. The texts that circulate in this way, however, are heavily skewed towards those written in English by US and British authors. If we continue to follow a British example, the major postprocessual texts that came out of British archaeology in the 1980s and 1990s, particularly the work of authors like Ian Hodder, Michael Shanks and Chris Tilley, have circulated very widely indeed. They were staples of the History and Theory of Archaeology courses I encountered in the Chilean, Canadian, Bolivian, and US universities which I visited during my larger ethnographic project. Hodder's book *Reading the past* (1986), for example, has been translated into Japanese, Spanish, Italian, Polish, Lithuanian, Greek, Macedonian, Chinese, Korean and Turkish. But, as I have demonstrated in this paper, these texts and the discussions of archaeological theory that they engage with come out of a specific British tradition of practice that has, embedded within it, particular understandings of the way in which archaeological knowledge is produced and how it can be known, and by whom. What happens to our understanding of these processes when the recording technology assumes that, in practice, the act of interpretation can be (and, for the sake of time on a busy four-week-long excavation, *ought* to be) separated from the act of physically engaging with the earth? When one writes about theory at the trowel's edge or a hermeneutic circle of data and interpretation, does this still make sense if the hand holding that trowel belongs to an indigenous Aymara *maestro* who is being told what to do in broken Spanish by a first-year graduate student holding only a pen? Moreover, papers produced by the directors or graduate students from the Andean project I studied could easily be published in the same journals as those produced by a British archaeologist like Hodder. None of these papers would include a discussion of their methods – whether, for instance, they used a Harris matrix and if so how and why – but in their textual form all could be seen as commensurable, in that each would be assumed to share the same ontological and epistemic foundation.

It is here that the larger epistemological consequences of black-boxing excavation methods become clearer. *Not* formally discussing excavation methodology in written texts means that both the variety in excavation methods and the reasons for choosing one technique over another, *and* the epistemological and ontological assumptions that underpin them, are potentially misunderstood as archaeologists from different communities evaluate each other's work through shared texts. Moreover, there is an underappreciation of the means through which archaeological knowledge is, in fact, produced through the embodied expertise and professional experience of individuals.

Whether undertaken by workers, volunteers or students – and no matter how they are organized – excavation always relies on human bodies to physically and conceptually bring into being its objects of knowledge. In other sciences, the bodily processes of making, identifying or isolating objects of

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knowledge can be mediated to a greater or lesser extent through technology: air-pumps, microscopes, computers and so on that serve to enhance, modify or objectify the vision and touch of the scientist (e.g. Shapin and Schaffer 1985; Daston and Galison 2007; Knorr Cetina 1999; forthcoming). In archaeological excavation, the most significant – if not the only – machine used is the machine-like organization of human labour. At various moments through this paper I have drawn attention to the tension that exists between what the system of excavating assumes about both people and objects of knowledge, and what is experienced in practice. This tension, I argue, derives from the mismatch between the idea that scientific knowing should be impersonal and the day-to-day experience on site that 'tools' are actual people.

In this sense, archaeology is an extreme version of what the historian of science Steven Shapin has described as an 'essential tension' in accounts of modern scientific authority. People and their virtues *do* matter to the making of scientific knowledge, but this seems like a 'perverse' claim:

> What does it mean to say that people matter? I mean that we cannot understand how various scientific and technological knowledges are made, and made authoritative, without appreciating the roles of familiarity, trust, and the recognition of personal virtues. And the reason such a claim may seem perverse is that both these knowledges and the means by which they are produced are widely accounted *impersonal* – having nothing to do with personal characteristics and patterns of familiarity and enjoying their special authority hrough *being understood* to have no such dependencies (Shapin 2008, 1, original emphasis).

Philosophers of science, and scientists themselves in their narrations of their own practice, state that in late modernity scientific authority comes from separating personal characteristics from the practice of science; Shapin demonstrates that this never has been the case, although the narrative of impersonal science has done very particular work at various points in time to support scientific authority both within disciplines and between scientists and the public. Archaeology has been no different in expecting the personal characteristics of individual excavators (the ability to touch, to see and to reason; to speak, debate and argue; but also their personal and professional 'virtues') to be something that ought to remain separate from the process of evaluating the knowledge that individuals produce.

But this is why excavation remains an area of concern and tension for individual archaeologists. Archaeology relies on the embodied expertise of individuals, but the common perception of 'good science' within the broader scientific community and among the general public is that science is impersonal. Paying more attention to how archaeologists do, in fact, produce knowledge in the field would require drawing attention to the centrality of embodied expertise to archaeological practice. I would suggest, however, that not only would breaking open the 'black box' of excavation allow us to address some of the problems that arise between archaeologists from different communities of practice, as alluded to above, but it would also result in a greater appreciation of the complexity and expertise involved in creating archaeological knowledge in the field.

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Notes

[1] In this paper I use the term 'scientist' as a catch-all to cover all these definitions and to include those working in the social, the natural and the human sciences. I use it while acknowledging the discomfort that some archaeologists feel with being termed 'scientists'. But I use it as an alternative to the somewhat cumbersome term that might best serve as a neutral alternative: 'institutionally recognized expert knowledge producer'.

[2] As a further illustration of this point, I noted during the course of my ethnography several examples of individuals who were successful and considered to be 'good archaeologists' despite having a reputation for running notoriously disorganized and badly excavated projects. The means by which data is acquired in the field – even when the methods and conditions for that data gathering *are* known – does not affect the perceived value or 'truthfulness' of subsequent claims in publications.

[3] The name of the project and all personal names in this paper are pseudonyms. This particular excavation project is typical for the region. The project itself is part of what I refer to as the 'Andeanist archaeological community', a subdisciplinary community of archaeologists based and trained in specific US and Canadian universities, who work in a geographic region whose borders are diffuse but roughly map onto the north of Bolivia, and the south, coast and certain highland areas of Peru, and who know each other as colleagues and friends. While the working practices, labour organization and methodologies of Andeanist archaeology are far less homogeneous and more likely to be modified than the British example, much is shared between different projects and as such this can be taken as a typical example.

[4] In Bolivia, the *licenciatora* is the highest professional degree awarded. It requires several years of specialized course work and an independent research project resulting in a thesis similar in length and depth to a British Ph.D. thesis. Although it shares the same name, it is not identical to a *licenciatora* in other South American countries such as Peru and Chile.

[5] Other countries in South America also have *contraparte* positions, but the degree to which this is a symbolic or an active co-directorial position varies from place to place.

[6] I refer specifically here to a single community (*el comunidad*), rather than to the town/municipality as a whole.

[7] In addition there was a sewing quad, responsible for making all the cotton draw-stringed bags that were used for storing artefacts, and four cooks who catered to the archaeologists' domestic life (i.e. cooking, cleaning and laundry).

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[8] The lab, sewing and flotation quads had one member from each *zona*. The cooks, meanwhile, were entirely separate. These women were among the highest-paid of all staff, and had in some cases over a decade of experience working for many of the North American archaeological groups in this region. Two new young women were employed in 2008, one of whom was the daughter of the facilitator. But the other two older cooks were from entirely different communities elsewhere in the Tiwanaku region and were highly sought after by North American archaeologists for their friendliness, experience and ability to cater to US diets.

[9] In other cases, as for instance on Kenyon's excavations in the early twentieth century, the diary is organized so that a single page corresponds to part of the excavation: a grid, or a feature per page (Lucas 2001, 56). In either case the page is presented as a blank for them to fill in as they deem fit.

[10] The UK is in fact remarkable for the homogeneity of its methods and the dominance of the Winchester system – or, as it is more commonly now known, the MoLAS recording system – across commercial and academic archaeology.

[11] *The invisible diggers* (2009), Paul Everill's in-depth study of the state of British commercial archaeology in the 2000s, comes from a position sympathetic to those who argue that context forms are democratizing, but draws attention to the larger structural problems of essentially vocational professionals working within a neoliberalized, commercial sector. Everill argues that chronically low pay, short-term contracts, poor working/living conditions and little room for promotion or advancement, when coupled with little acknowledgement of excavators' contributions to final reports and publications, mean that excavators are indeed invisible and thus barely better off than the labourers employed by Wheeler in the 1930s. Although he wishes to draw attention to and celebrate the centrality of the excavator in the production of archaeological knowledge, Everill's work reminds us that this needs to be balanced with a conversation about the lack of actual socio-economic and epistemic power that excavators currently have. At the same time, it is interesting that the conversation about 'democratizing' archaeology has shifted in the decade since these articles were written. Today the discourse of 'democratizing archaeology' in the UK increasingly signifies a return to relying on the labour of unskilled, unpaid volunteers who are enthusiastic members of the public (where democratizing = letting the public be involved in uncovering the past), rather than on skilled, *paid* excavators using single-context planning forms.

References

Anderson, B., 1983: *Imagined communities. Reflections on the origin and spread of nationalism*, London.

Berggren, A., and I. Hodder, 2003: Social practice, method, and some problems of field archaeology, *American antiquity* 68, 421–34.

Berrett, D., 2010: Anthropology without science, *Inside higher education*, 30 November 2010, available at www.insidehighered.com/news/2010/11/30/anthroscience, accessed 10 March 2015.

Collins, H.M., 2001: Tacit knowledge, trust and the Q of sapphire, *Social studies of science* 31, 71–85.

Collis, J., 2001: Digging up the past. *An introduction to archaeological excavation*, Stroud.

Daston, L.J., and P. Galison, 2007: *Objectivity*, New York.

Edgeworth, M., 2003: *Acts of discovery. An ethnography of archaeological practice*, Oxford.

Everill, P., 2007: British commercial archaeology. Antiquarians and labourers, developers and diggers, in Y. Hamilakis and P. Duke (eds), *Archaeology and capitalism. From ethics to politics*, London, 119–36.

CAMBRIDGE JOURNALS
http://journals.cambridge.org Downloaded: 18 May 2015 IP address: 129.105.135.124

Everill, P., 2009: *The invisible diggers. A study of British commercial archaeology*, Vol. 1, Oxford.

Farid, S., C. Cessford, A. Berggren, A. Cooper, R. Turnbull, A. Baysal, S. Leaver, P. Boyer, T. Cawdron, N. Hamilton, L. Hawkes, M. Knight and S. Webb, 2003: The excavation process at Çatalhöyük, in I. Hodder (ed.), *Towards reflexive method in archaeology. The example at Çatalhöyük*, Cambridge, 19–35.

Harris, E.C., 1989: *Principles of archaeological stratigraphy*, 2nd edn, London.

Harris, E.C. and P.J. Ottaway, 1976: A recording experiment on a rescue site, *Rescue archaeology* 10, 6–7.

Hodder, I., 1986: *Reading the past. Current approaches to interpretation in archaeology*, Cambridge.

Holtorf, C., 2002: Notes on the life history of a pot sherd, *Journal of material culture* 7, 49–71.

Holtorf, C., 2006: *Archaeology is a brand! The meaning of archaeology in contemporary popular culture*, Oxford.

Knorr Cetina, K., 1999: Epistemic cultures. *How the sciences make knowledge*, Cambridge, MA.

Knorr Cetina, K., forthcoming: The mediatization of the face-to-face situation. Scopic media and global coordination, in K. Lundby (ed.), *Handbooks of communication science*, Vol. 21, *Mediatization of communication*, Berlin.

Latour, B., and S. Woolgar, 1979: *Laboratory life. The social construction of scientific facts*, Beverly Hills, CA.

Leighton, M., 2014: Uneven fields. Transnational expertise and the practice of Andean archaeology, Ph.D. dissertation, University of Chicago.

Lucas, G., 2001: *Critical approaches to fieldwork. Contemporary and historical archaeological practice*, London.

Sammells, C., 2013: Complicating the local. Defining the Aymara at Tiwanaku, Bolivia, *International journal of historical archaeology* 17(2), 315–31.

Shanks, M., and R.H. McGuire, 1996: The craft of archaeology, *American antiquity* 61(1), 75–88.

Shapin, S., 2008: *The scientific life. A moral history of a late modern vocation*, Chicago.

Shapin, S., and S. Schaffer, 1985: *Leviathan and the air-pump. Hobbes, Boyle, and the experimental life*, Princeton, NJ.

Smith, M.E., 2010: Anthropology is not a science – American Anthropological Association, available at http://publishingarchaeology.blogspot.com/2010/12/anthropology-is-not-science-american.html, accessed 22 October 2014.

Swartley, L., 2002: *Inventing indigenous knowledge. Archaeology, rural development, and the raised field rehabilitation project in Bolivia*, New York.

Tilley, C., 1989: Excavation as theatre, *Antiquity* 63, 61–81.

Wood, P., 2010: Anthropology Association rejecting science, *Chronicle of higher education*, 29 November 2010, available at http://chronicle.com/blogs/innovations/anthropology-association-rejecting-science/27936, accessed 10 March 2015.

Yarrow, T., 2003: Artefactual persons. The relational capacities of persons and things in the practice of excavation, *Norwegian archaeological review* 36, 65–73.

Yarrow, T., 2006: Sites of knowledge. Different ways of knowing an archaeological excavation, in M. Edgeworth (ed.), *Ethnographies of archaeological practice. Cultural encounters, material transformations*, Lanham, MD, 20–32.

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